New Evidence on the Casual Effect of Traffic safety Laws on Drunk Driving and Traffic Fatalities

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Abstract

In the United States, about 28 lives are lost daily in motor vehicle accidents that involve an alcohol-impaired driver. The conventional wisdom is that these accidents can be prevented through the use of strict traffic laws that are robustly enforced, though no consensus exists on the causal impact of these laws in reducing motor vehicle-related fatalities. This paper exploits quasi-random variation in state-level driving and road safety restrictions to estimate the causal effect of select traffic laws on the number of fatal accidents and fatal accidents involving a drunk driver. In this paper, we employ the contiguous-border county-pair approach. This is causally identified from the discontinuities in policy treatments among homogeneous contiguous counties that are separated by a shared state border. This approach addresses the econometric issues created due to spatial heterogeneity that may have biased several studies in the literature. The analysis reveals that the laws related to accident prevention, such as having a good graduated licensing system, pigovian beer taxes and primary seatbelt enforcement, are the most effective in reducing motor vehicle-related fatalities. Using these estimated coefficients, simple simulations suggest that policymakers have been utilizing existing traffic laws sub-optimally, saving only 17% of the lives lost to motor vehicle crashes.

Keywords: traffic-fatalities, drunk-driving, traffic safety legislations

1 Introduction

In the United States, having access to a motor vehicle has become one of the essential conveniences of modern life. From 1990, survey estimates have consistently shown over 90% of households having access to this commodity. Operating a motor vehicle, however, is not without its fair share of risks, evident by the National Center for Health Statistics (NCHS) ranking motor vehicle accidents as the number one leading cause of death among teens and the 13th leading cause of deaths in the U.S in 2013. This hazard is compounded by risky behavior by some economic agents, such as speeding or driving while impaired. The National Center for Statistics and Analysis (2015) estimates that across all states in 2014, alcohol-impaired-driving fatalities accounted for between 20 percent (Vermont) and 41 percent (Massachusetts, North Dakota, and Texas) of the total count of traffic fatalities, with a national average of 31 percent. On a daily basis, there are 28 recoded deaths in motor vehicle accidents that are due primarily to an alcohol-impaired driver, with one of such loss of life occurring every 53 minutes. This also carries an estimated annual cost of $44 billion dollars resulting from lost productivity, legal, court and medical related expenses, property damage and insurance administration. Consequently, the issue of traffic safety is a major health concern not only to those directly involved, but to society as a whole.

The conventional wisdom is that these outcomes can be prevented through the use of strict traffic laws that mandates safe motor vehicle operation, adequately punish offenders and are robustly enforced. As a result, traffic laws have played a dual function of both maintaining the orderly flow of traffic as well as creating an incentive structure which dissuades individuals from engaging in any activity which could potentially lead to harm. Policy makers have attempted to legislate how vehicles are used on public carriageways, including placing speed limits, mandating seatbelt use and restricting licenses among other measures. Many states have also implemented measures aimed at discouraging certain behavior by targeting those that are most likely to become perpetrators. For example, some states have implemented beer taxes aimed at making alcohol consumption more expensive; reducing excessive drinking and essentially passing through the cost of the negative externality created through alcohol consumption to those who cause the externality. At the extensive margin, a law like this increases the cost associated with alcohol consumption and as such should deter some individuals from drinking or at the very least reduce the quantity of alcohol individuals consume. If this pigovian tax is effective, then it should reduce the level of inefficiency created by alcohol consumption and as such should reduce the frequency of individuals being at risk by driving while intoxicated.

This study exploits the across state variation in traffic laws to establish the causal impact of state-level legislation on traffic and drunk driving related fatalities. We argue that the implementation of and/or removal of pigovian taxes, historical laws, and traditional legislative restrictions creates a perfect natural experiment that can be used to causally identify the effect of legislative driving and alcohol restrictions on the number of traffic related fatalities. This is done within a contiguous county specification which controls for spatial heterogeneity. By restricting our analysis to laws that are quasi-random and our sample to county pairs that share the same border, we are able to exploit the policy discontinuities at the state border to identify the effect of driving and alcohol restrictions, keeping only the variation within each across state contiguous county pair. The contiguous counties identification approach utilized in this paper follows the earlier studies by Huang (2008) and Dube et al. (2010). In this paper, we present several specifications tests and convincing evidence of the causal relationship that we seek to establish.

In our preferred specification, we found that the laws that are directly related to accident prevention and safety, such as having a good graduated licensing system, pigovian beer taxes and primary seatbelt enforcement, are the most effective policy tools to address the issue of traffic fatalities. Through simple simulation techniques using the estimated parameters, we show that available legislative solutions to this problem have been under utilized by policymakers over our sample period. Consequently, our results suggest that these tools can be used to this widespread public health issue if they are employed at the federal level.

The remainder of this paper is outlined as follows: Section II provides a brief overview of the related literature. Section III describes the data we utilized, the model specification and provides an outline of the identification strategy utilized. Section IV provides the empirical estimates and robustness checks. Section V outlines the policy implications and the major conclusions derived from this study.

2 Literature

The everyday significance of traffic accidents and traffic-related fatalities is reflected in the plethora of studies that have been conducted to assess the impact of traffic laws on traffic safety outcomes. The robustness of these studies have often been questioned due to the variances in results and a lack of a coherent narrative across the studies. The differences in methodology, mainly the extent to which spatial heterogeneity is accounted for, and variation in sample sizes, may have contributed to this lack of consistency. For the purposes of this review, we group previous studies based mainly on the nature of the laws rather than the methodology or chronological order of the studies. This helps to highlight the variation in the results that occur when the methodology and datasets are adjusted. These studies have assessed the impact of alcohol control legislations (Freeman, 2007; Foss et al., 2001; Fell and Voas, 2006; Chaloupka et al., 1993), license laws (Fell et al., 2011; Shope, 2007; Williams and Shults, 2010), driving restrictions and vehicle operation rules (Ossiander and Cummings, 2002).

Alcohol control legislations can be divided into two categories, namely preventive or ex-post regulations (Ying et al., 2013). On the one hand, preventative regulations seek to limit the public's access to alcohol by increased prices (tax laws), limiting access (blue laws) and restricting possession (open container laws). On the other hand, ex-post laws seek to punish those that are guilty of driving while impaired (DWI) by establishing minimum punishments, gradations of guilt and licensing restrictions. The effectiveness of these regulations is not consistent across the literature, with variations in data and methodology predicting varying effects.

The estimated impact of the BAC law on traffic fatalities provide just one example of this inconsistency found in the literature. While some studies suggest that BAC laws, specifically the lowering of the BAC threshold, do not have a significant impact on traffic fatalities (Freeman, 2007; Foss et al., 2001), others find that this law can be meaningfully used as policy lever to address this phenomena (Fell and Voas, 2006; Eisenberg, 2003; Wagenaar et al., 2001; Dee, 2001; Hingson et al., 2000). The differences between the results of these studies is attributed to inconsistencies in the time period employed (Freeman, 2007) and the use of intra or inter-state comparisons (Foss et al., 2001).

Early studies into the effectiveness of beer excise taxes suggested that they were very effective in reducing traffic fatalities, a narrative that has not been consistently supported by later studies. The earlier studies found that increasing the beer tax rate reduced the total level of fatal traffic accidents across all age groups (Cook, 1981; Saffer and Grossman, 1987). These studies were subsequently challenged on the basis that they did not adequately address the issue of unobserved heterogeneity (Dee, 1999). More recent studies have suggested that increasing the beer tax either have a small effect (Chaloupka et al., 1993) or an insignificant impact (Mast et al., 1999; Young and Likens, 2000; Young and Bielinska-Kwapisz, 2006) on traffic fatalities.

The effectiveness of DUI fines and Administrative License Revocations (ALR) have been explored by many studies in the literature. The estimated impact of this law have varied profusely across states and studies. As such, while some studies have found DUI fines to be effective in reducing traffic fatalities (Chaloupka et al., 1993; Wagenaar et al., 2007; Wilkinson, 1987), other studies find dissimilar results (Young and Likens, 2000). Mixed results have also been found for ALR laws (Ying et al., 2013).

Laws that seek to improve traffic safety and reduce the risk of an accident by regulating how vehicles are operated, such as seat belt and speed limit legislations, are often found to negatively associated to traffic fatality rates (Freeman, 2007; Cohen and Einav, 2003). However, there is a lack of concurrence on the magnitude and significance of these effects. Cohen and Einav (2003) point out that the rate of effectiveness of seat belt laws is overestimated by some studies and government policy makers. They posit that more accurate data, in terms of percentage of seat belt usage, and differentiating between primary and secondary enforcement allows for a clearer picture of the effectiveness of seat belt laws. They also point out that the ability of their study to exploit interstate variation in seat belt laws allowed for more accurate modelling of the effect of seat belt laws, while previous studies had relied heavily on Intra-state level analysis impacting the estimated parameters.

The dependency on intrastate level analysis and data is a feature of most studies in the field. Time series and panel anaysis is often used to assess the effectiveness of a particular policy change. While it is possible to control for some relevant factors, these methodology are particularly prone to the adverse influence of unobserved heterogeneity arising from spatial differences in individual behavior that may be correlated with traffic legislation policy changes. This can also be seen in the research of Graduate Driver's License laws.

Given the high propensity of young drivers to be involved in fatal crashes, there has been an effort to extend greater control on how this cohort operates motor vehicles. Graduate Driver's License (GDL) laws encompass a number of measures that seek to restrict vehicle operation by young individuals. The extent to which and the type of GDL laws implemented by states vary widely and the significance of their impact on fatal accidents has varied across states and studies as a result (Fell et al., 2011; Shope, 2007). Despite these variances, there is a consensus in the literature that GDL laws are effective in reducing the rate of teen involvement in traffic accidents and fatal crashes, specefically for 16 and 17 year olds (Chen et al., 2006; Fell et al., 2011; Shope, 2007). There is, however, evidence that the benefits of such laws do not extend past the immediately impacted cohort (Chen et al., 2006) and even a suggestion that strong GDL laws were associated with increased involvement in fatal crashes by the 18 year old cohort (Masten et al., 2011). There remains a need for a more comprehensive analysis of the average effect of these laws across the population, over a prolonged period of time. Additionally, there is a gap in the literature in examining the differential impact of the GDL law on drunk driving fatalities.

Despite the plethora of related studies within the field, there remains a high level of inconsistency in the results which warrant further investigation. The tendency for studies to utilise state level panels and time series data makes their findings susceptible to issues of spatial heterogeneity and omitted variable bias. By utilising the contiguous counties model a more natural control group is utilised while helping to limit the effects of spatial heterogeneity. This should yield estimates that are more consistent and reliable.

3 Data and Empirical Specification

In this section, we provide a general overview of the data, selection restrictions and methods we employ in the paper. We begin with a description of the key sources of the data in section 3.1, sample restrictions in section 3.2, descriptive statistics in 3.3 and then concluding with the empirical model specification in section 3.4.

3.1 Data

To be consistent and for ease of comparability with the previous studies in the literature, we utilize data on traffic fatalities from the Fatality Analysis Reporting System (FARS). This dataset is compiled by the National Highway Traffic Safety Administration (NHTSA) using data reported by various state level agencies. It is comprised of fatal accidents involving a motor vehicle being used on any traffic-way that is open to the public within any of the 50 States and the District of Columbia. To qualify for inclusion, the incident must have resulted in the death of an involved party within 30 days of the crash.

The quality of this data should be reasonably high, since coding manuals are provided with a set of instructions on how coders can transfer information from a police accident/crash report to the FARS; several classes are held annually to train coders and system wide meetings are held to encourage uniform coding practices. The data is collected in a very organized manner, with several regional NHTSA offices and several trained FARS analysts collecting, transcribing and transmitting their state's data to the centralized system in a standard form. These analysts consult several records including; death certificates, coroner/medical examiner reports, hospital records, vital statistics police reports and the state highway department data. For each county, we begin by aggregating the total number of fatal accidents involving a drunk driver and the total number of overall fatal accidents.[1] To check the robustness of our estimates and to appropriately address the potential concerns of our readers, we also provide estimates from models involving these outcomes calculated for various days of the week and times of a given day.

We also utilized data on state traffic laws from the State Health Policy Research Dataset (SHEPRD). This dataset was created to examine state level public health laws from 1980-2010 and features laws such as beer taxes, minimum and maximum fines and other forms of traffic

[1]It should be noted that the county observed in the FARS dataset is the location where the accident occurred and not the driver's county of residence. This raises concern of a potential migration bias. This is more extensively discussed in section 3.5

rules such as seatbelt and graduated license laws. Compilation of the data set was facilitated by the Robert Wood Johnson Foundation and the United States Department of Health and Human Services, the National Institute of Health and the National Institute on Alcohol Abuse and Alcoholism. We focus on the period 1986-2005, since the majority of the changes in traffic laws occurred during this period and since this time period minimizes the number of missing laws we have in our dataset. This results in a balanced dataset consisting of traffic laws and outcomes over a 20 year period, for 49 states.

Finally, to ensure we are adequately accounting for the size of the county in our analytical framework, we collect county-level population data from the US Census Bureau and land area data from Dube, Lester and Reich (2010) to complete our dataset. Given the method we employ, we have compiled one of the largest aggregated dataset to be used in the literature to date, which should also lead to an efficiency advantage over previous studies[2].

3.2 Sample Restrictions

The balanced dataset utilized in this study was comprised of observations at the county level over the period 1986-2005. The contiguous border counties sample (CBCS) is further restricted using the population sizes and land area disparities within each contiguous county pair. Given our criticism of the heterogeneous comparison groups utilized in previous studies, this study further restricts the sample by dropping county-pairs where the difference in the size of the land area (in squared miles) and population within the pairs are too large. This saw county-pairs having the top 5% of disparity in land area or population being omitted from the sample. Consequently, for each pair within this sample, we calculate the within pair absolute difference in the land area and population counts. The county pairs belonging to the tail (defined as being beyond the 95th percentile) of the resulting distribution are omitted from our analysis. This relies on the plausible assumption that county-pairs that are comprised of counties with similar land area and population size are more homogeneous. This prevents us from comparing very big counties (either in land area or population) to very small counties that are by nature less similar. Through the use of contiguous county-pairs across state lines, eliminating county pairs where the within pair variation in land area and population are outliers and by using county-pair by year fixed effects, this study has employed the largest combination of measures to address the issue of spatial heterogeneity (common time trend) that studies in this literature have utilized to date. Finally, we omit Hawaii from our analysis for obvious reasons.

3.3 Descriptive Statistics

The restrictions imposed on our CBCS reduces the disparities in the observed characteristics within each pair by design. This can easily be seen from table 1, where the standard deviations from the contiguous counties sample are sufficiently smaller than those from the sample including all counties, though the mean values from both samples are very similar.[3]. Across both

[2]In the first draft of this paper, we also included estimates based on the county level DUI counts, from the Uniform Crime Reports (UCR) database published by the Federal Bureau of Investigations (FBI). However, due to issues of faulty reporting with this data and the estimates from all specifications raising serious concerns, we omitted these estimates from this version of the paper.

[3]This conclusion does not hold for the population and land area variables, since these were used to restrict our CBCS. Therefore, the two samples will be different along these dimensions by design

samples, the average number of fatal crashes and fatal crashes involving an intoxicated driver was approximately 26 and 11 per hundred thousand population respectively.

Table 1: County Level Descriptive Statistics 1986-2005

	Contig-Counties Sample		**All-Counties Sample**	
	Mean	Std. Dev.	Mean	Std. Dev.
Number of Fatal Crashes	25.78	24.74	25.67	27.33
Number of Drunk Fatal Crashes	10.84	14.82	10.72	17.89
GDP Per Capita	27573.07	7791.85	27996.28	7822.51
Poverty Rate	13.32	3.75	13.38	3.61
Motor Vehicle Reg. in 000s	4493.73	3763.26	5470.53	4677.99
Population	56371.44	101678.20	84729.59	279100.10
Land Area (square miles)	1000.40	1230.58	1114.36	3472.10

While these descriptive statistics do not provide conclusive evidence that the CBCS solves the problem of spatial heterogeneity, this table offers two key insights. First, since our preferred specification is identified from the variation in traffic policies within each contiguous border county pair, it is necessary that the counties observed characteristics within each pair are similar. Through our restriction of omitting pairs where the difference in population or land area is 'too large', we are able to reduced the standard deviation in the observed characteristics across all pairs. Since the deviation from the mean characteristics in the CBCS is noticeably smaller than in the sample including all counties, this suggests that there is a higher degree of homogeneity in the former. Second, the restrictions we imposed at the pair level and the use of only contiguous counties led to very large counties (in terms of population size and land area) being omitted. This is necessary since these counties are unlikely to be similar in observed and unobserved characteristics to other counties. Both of these observations offers moderate support to the identification framework we exploit. However, given the multi-dimensional space of the policies/treatment we are evaluating, we are unable to offer stronger evidence of comparability.

Table 2 provides a summary of the policies we examine in this study. Since the identification framework utilized in this paper relies on the changes in policies within each contiguous county pair, this table shows the mean value for these laws across all states and the number of states that have changed each of these traffic rules over the period of study.

For each discrete variable (e.g. a law prohibiting open containers in cars), we provide the number of states who have changed the law over the period. For the continuous variables (e.g. state beer tax), we present the change in the mean value of the variable from 1986 to 2005. As we can see from the table, there is sufficient variation in state traffic legislations over the period to identify the model we present below. The only laws which offer a cause for concern are mandated prison time for drivers committing their first DUI and potentially the seatbelt law for minors. Therefore, since we do not observe much changes in these variables overtime, we must be cautious in explaining the estimated parameters from the model for these policies.

3.4 Empirical Model Specification

3.4.1 Traditional Specification

Previous studies have predominantly utilized a panel of states to evaluate the effect of traffic legislations in explaining traffic fatalities. The results obtained by these studies are potentially

Table 2: **State Level Descriptive Statistics**

	Mean	Std. Dev.	**Change 2005-1986**
Marginal GDL	0.15	0.36	14
Fair GDL	0.13	0.34	19
Good GDL	0.05	0.21	12
Law Prohibiting Open Containers in Cars	0.50	0.50	27
Beer Keg Registration Required	0.25	0.43	22
State Beer Tax ($ per Volume)	0.23	0.17	0.05
BAC adult drivers	0.09	0.01	-0.02
Law Restrict Cig. Ads	0.17	0.38	15
Ban smoking in bars	0.04	0.20	7
Ban smoking indoor restaurants	0.05	0.23	10
Tax per pack of Cigarettes ($)	0.36	0.31	0.66
Mandated Comm. Service 1st or 2nd DUI	0.38	0.49	-12
License Susp. Pre-Con. 1st DUI (days)	0.63	0.48	21
Mandated prison time 1st DUI	0.33	0.47	1
Mandated Min Fine 1st DUI	139.35	186.01	131.63
Speed limit rural interstate	66.03	6.29	14
Underage BAC Limit of 0.02 g/dl	0.57	0.49	45
Bicycle Helmet Use	0.21	0.40	18
Seatbelt Law Minors	0.04	0.20	3
Seatbelt Law All Drivers	0.23	0.42	14
Seatbelt Fine	0.82	0.38	0.71

biased since they utilize econometric specifications which typically compare the fatality rates in states that have enacted a particular traffic law(s) to those that did not. These approaches assume that each state legislature's decision to implement these laws are independent of all omitted factors which potentially influences individuals' decision to drive while intoxicated or the count of fatal accidents in the state. Consequently, the extent of the bias will depend on the degree to which the pre-implementation trend in fatality counts in the state(s) which enact various traffic laws differ from those who do not implement these laws and the ability of previous studies to include variables which control for all the factors driving these differences. Due to the high degree of heterogeneity between the states in these samples, the usual practice of including time and state fixed effects yield little additional benefits in providing estimates that can be interpreted causally. Other studies in the literature have utilized time series or ARIMA intervention models (insert such examples here) which may suffer from similar issues of model misspecification or changing trends in fatalities over time. Time series studies also rely on the assumption that the intervention is exogenous, which is usually not satisfied in this environment. While a majority of the previous studies provide us with key descriptive insights in understanding the impact of traffic rules on driving related fatalities; there is still a need for robust estimates produced using a method which address the limitations pointed out in the previous literature. We propose that the method utilized in this study may be able to bridge this gap in a meaningful way. We begin by estimating models which replicates the previous studies using several methods employed in previous works.

We begin with the model that is most often utilized. This specification can be expressed as follows:

$$F_{st} = \alpha + T_{st}\pi + X_{st}\phi + \theta_s + \theta_t + \varepsilon_{st}$$

Where F_{st} is the number of total fatalities or fatalities involving a drunk driver in state s at time t, T is a vector of traffic laws, θ_s is state level fixed effect and θ_t are time fixed effects. To test this approach robustly, we also include other state level controls that potentially explain the differences in the fatality trends observed across states and also may be correlated with the timing of the implementation of traffic laws. However, under even the most robust specification of this model, there is a high likelihood that the necessary condition $E(\varepsilon_{st}|T_{st}, X_{st}) = 0$ is not satisfied due to the high degree of heterogeneity which exists among states. Before presenting our most preferred empirical specification, we also show that utilizing all counties in the united states would also provide estimates that are expectedly unreliable. This is because we are still comparing very distinct groups, each possessing a time trend in the outcome variable that may be distinctive. Therefore, the differences in the time trend between counties pre- and post-interventions are likely to lead us to conclude a false positive in judging the effectiveness of the intervention. The specification we are using for the all counties sample is:

$$F_{ct} = \alpha + T_{st}\pi + X_{st}\phi + \theta_c + \theta_t + \varepsilon_{ct}$$

Identification in this model requires that $E(\varepsilon_{ct}|T_{st}, X_{st}) = 0$, which is not satisfied. There are a number of unobservables that are potentially correlated with T and also affect driving fatalities. Therefore, differences in wealth, education or even racial composition could easily violate this exogeneity of treatment assumption that is required to estimate causal parameters.

3.4.2 Identification from Discontinuities at Contiguous State Borders: Contiguous Counties Approach

Our preferred specification proposes a more generalized difference in difference strategy which exploits the variation in adopted traffic laws between contiguous counties pairs across border states to causally identify our parameters of interest. In advancing this approach in the current literature, we cautiously subscribe to the view that a contiguous county method utilizes more homogeneous control groups than approaches which employ a contiguous states approach, those just randomly comparing states in general or any approach utilizing all counties in the US. Additionally, this panel approach allows us to difference out between county-pair variation which may affect the outcome. By including county-pair specific fixed effects, we identify our results using only the variation in the policies of interest which occur within the thousands of contiguous county pairs we utilize. That is, this approach relies on the changes in traffic legislation which occurs within each across-state contiguous county-pair for identification of the parameters. As such, the identifying assumption is that policy differences within county-pairs are uncorrelated with differences in residual driving fatalities in either county.

Another main benefit of using this approach is that the county pair specific time trend absorbs the unobserved heterogeneity that previous approaches have ignored. The preferred specification using this approach is given by:

$$F_{cpt} = \alpha + T_{st}\pi + X_{st}\phi + \theta_c + \nu_{pt} + \varepsilon_{cpt}$$

Where c, p, t indexes counties, pairs and time; ν_{pt} is the county-pair by year fixed effects and our outcome and errors are now defined at the county-time level within each contiguous county pair extending across a state border. Under this specification,a causal interpretation is more likely to be achieved as the assumption $E(\varepsilon_{cpt}|T_{st}, X_{st}, \nu_{pt}) = 0$ is now satisfied.

3.5 Main Threats to Causality: Migration Bias

Estimating the true effect of a policy becomes troublesome when the treatment is based on geographical location and individuals can migrate either to avoid or to be included in the treatment. In the context of our study, this would be a problem if there exists sufficient migration across state borders in response to changes to traffic legislations such that the counties within each contiguous county-pair becomes highly heterogeneous. Consider two contiguous counties, A belonging to state X and B belong to state Y. Let us assume that both counties are identical in terms of their observed (including their traffic laws) and unobserved characteristics. Let us suppose that state X decides to move to a system of secondary enforcement of it's seatbelt law and state Y continues with their primary enforcement seatbelt law. A researcher exploiting this change in policy to estimate the impact of seatbelt enforcement on traffic fatalities would produce coefficients affected by the migration bias if this policy change induces the riskiest individuals who are most prone to accidents in county B to move to county A. This movement of individuals across the state border due to the change in policy leads to the counties no longer being identical as is required. We strongly believe that this is not a major concern in this paper due to the high direct and psychic cost associated with migration. Therefore, the share of individuals induced to relocate to nearby states due to a change in traffic rules is infinitesimal and should pose no threats to the identification framework we employ.

There is also a likelihood that the county where the fatal accident occurs and is counted in our data is different from the driver's county of residence. This only poses a problem if it is done in a systematic manner which inflates the fatality counts for one of the contiguous county within a given border pair. Since the location where a fatal accident occurs is likely a random event, we do not expect accidents occurring outside of the driver's county of residence but picked up in our sample to violate our identifying assumptions. We conduct several robustness checks to show that the second case is unlikely to bias our main results. By partitioning our data into several sub-samples, we are able evaluate how our coefficient changes when the composition of the drivers who are likely to be on the road varies.

4 Empirical Findings

In this section, we present results from the main conventional specifications utilized in the literature as well as the contiguous border county-pair specification we propose in this study. In all models we include; smoking legislations (laws restricting cigarette ads, bar and indoor smoking bans and cigarette tax), the state's GDP per capital, land area, log of motor vehicle registration, the governor's political party affiliation and several measures of the ideological leanings of the state as control variables in our analysis. However, our analysis focuses on the impact of laws directly related to traffic safety and drunk driving prevention.

4.1 Traditional Specifications Estimate

We begin the empirical analysis by estimating the effect of traffic laws on driving related fatalities using the traditional specifications. These results are presented in tables 3 and 4.[4] Panels one and five show the results for all traffic fatalities and for fatalities involving a drunk driver using the state level specification. Panels two and six then present the results of the traditional specification including all counties in the United States. We control for state fixed effects in the state level specifications in panels one and five; county fixed effects in the all-counties specifications in panels two and six and year fixed effects in all panels presented in table three and four. Finally, for all the traditional estimates, we cluster the standard errors at the state level.

Relying on the estimated parameters from the state-level specification in one and five would lead to the conclusion that many of the existing traffic legislations are effective in reducing total traffic fatalities and the number of fatalities involving a drunk driver. Panel one estimates suggest that states with stringent alcohol tolerance laws and non-pecuniary penalties are associated with lower levels of total accident related fatalities per one hundred thousand population. These include enacting a good graduated licensing system, suspending drivers' license after they commit their first DUI, mandating lower BAC limits and imposing laws legislating bicycle helmet and seatbelt use. Model 5 also found that these laws were effective in addressing the rate of fatal accidents involving a drunk driver. For the state level specifications, the speed limit, open container laws, keg registration, beer tax rate, community service or prison time for DUI and pecuniary penalties were found to be ineffective in curbing driving related fatal accidents. Nonetheless, most of these laws had the expected negative impact on the rate of fatal accidents, but they also carried wide confidence intervals owing to sizeable standard errors. However, the state-level estimates are unreliable because of two key reasons: a) There is credible evidence against the exogeneity assumption being satisfied in these specifications and as such the estimated parameters are biased in a manner we cannot predict b) The standard errors from these models are unreliable not only due to model misspecification, but also because of the small sample size being utilized.

As expected, the estimates from the all-counties models in columns two and six are consistent with those discussed above and those found in the literature because of the high degree of spatial heterogeneity present in the sample. The coefficient from these models makes a compelling case for the conventional wisdom of using traffic rules in reducing the number of motor vehicle accidents. This is because a large subset of the traffic policies are statistically significant in influencing the total number of motor vehicle fatalities as well as motor vehicle fatalities involving an intoxicated driver. Under the all counties specifications, an increase in beer taxes reduces both types of motor vehicle fatalities. The rural interstate speed limit also becomes significant when the all-counties sample is utilized, relative to when the analysis is done at the state level. The effects of pecuniary traffic laws such as the maximum fine for seatbelt (SB) non-use, mandatory minimum fines for first DUI and laws reducing awards for injuries for seatbelt non-use remains statistically insignificant in these models. Additionally, other forms of punishments such as prison time or community service for the first DUI offence are still shown to be ineffective in addressing driving related fatalities. While the all-counties sample solves

[4] We evaluate the estimates in table 3 and 4 against a one-tail hypothesis due to the strong apriori theoretical that each law should either have no impact or a negative impact on driving fatalities

the sample size problem highlighted in b) above, we still expect the parameters from this model to be biased due to the high degree of heterogeneity among counties included in the sample. In most cases, the state-level estimates show a smaller impact than the estimates from the all-counties sample. However, because of our inability to sign the bias, we are unable to judge how the estimates from either model deviate from their true parameters.

4.2 Contiguous Counties Specification

Turning our attention to our main contribution to the literature, models 3, 4, 7 and 8 present the estimates from the contiguous counties specification. Panels 3 and 7 present a baseline specification that does not control for the unobserved time varying factors within each contiguous counties border-pairs. Panel 7 and 8 then introduce the county pair specific time trend which restricts our empirical identification to the variation in traffic laws which occurs within each border-pair. We present panels 7 and 8 as our preferred specification, since it addresses the main concerns we hold about the approaches common in the literature.

Restricting our focus to the contiguous county-pairs sample in panels 3 and 7, the qualitative conclusions of the various laws remain the same. However, the estimated parameters portray a stronger impact of each traffic law. This suggests that the spatial heterogeneity that the state-level and all-county samples suffered from induced an upward biased in the estimates. However, while using contiguous county pair reduces the degree of the heterogeneity between counties, the specification in 3 and 7 only corrects for the fixed unobserved county-specific and time-specific factors. As such, the unobserved time varying factors correlated with the implementation of various traffic legislations may result in endogeneity. To address this issue, we include a county-pair specific time trend in the results presented in panels 7 and 8. This is identified from the assumption that differences in traffic legislation within each contiguous county-pair is uncorrelated with the differences in the residual levels of driving fatalities between the counties in each pair. Under the preferred specification, we find that a fair graduated licensing system, open container, beer tax rate, BAC limits, bicycle helmet and a law reducing compensation for seatbelt non-use are all ineffective in reducing total driving fatalities though the traditional specifications are biased towards finding an impact. In contrast, we find that the open container law, beer tax and license suspension or prison time for first DUI are significant in explaining drunk driving related fatalities, while the traditional specification shows mixed results. If our identification assumption holds this result indicates that the traditional specifications are adversely affected by spatial heterogeneity or omitted variables bias that may lead to misleading conclusions about the effectiveness of each traffic law.

Additionally, while the qualitative conclusions across all specification are largely consistent, the point estimates varies profusely from one estimation procedure to the next. In particular, our preferred specification usually produces larger estimates than the traditional estimates reproduced in panels 1,2,5 and 6 respectively. In general, the contiguous-border county-pair parameter estimates suggest that the most effective traffic legislations in reducing driving related fatalities are the non-pecuniary measures directly related to alcohol use, drunk driving or accident prevention, such as open container law, beer tax, license suspension or prison time for the first DUI incident, alcohol limits for young drivers and seatbelt laws. This suggest that a mixture of preventative and ex-post laws can be effective in limiting the incidents of drinking and driving. In particular, the result highlights the effectiveness of the beer tax in forcing individuals to internalise negative externalities created when they drink and drive. In contrast, the results suggests that drivers do not change their risky behavior preferences in response to pecuniary penalties. This is supported by the insignificant result found for the minimum fine and the law reducing compensation for seat-belt non-use. However, an excise sin tax per volume of alcohol consumed disincentives individuals from engaging in risky behaviour that may result in traffic fatalities.

4.3 Robustness Checks

The main result questions the validity of the estimates in the literature. While it may be easy to convince readers that any estimation procedure that fails to account for the issues of spatial heterogeneity or omitted variables would yield parameters that are biased, it is not as easy to convince them that the contiguous county-pair specification adequately addresses this issue. To make the case for the empirical design we employed in this study, we present falsification tests in this section to show the robustness of the main results. To achieve this, we focus on time periods where we expect particular laws to have a predictable impact on individual behavior. Particularly, we examine the impact of traffic legislations in preventing drunk driving fatalities during Fridays and Saturdays 12AM to 3AM, Holidays (Fourth of July, Thanksgiving and New Years Day) and rush hour periods (Monday to Thursday from 3PM to 6PM). The first and second period of interest has the most notable spikes in the level of drunk driving fatalities. During these periods, we have a highly selective sample inclusive of riskier drivers who are more likely to be intoxicated and less likely to respond to traffic laws. That is, when we restrict our attention to a time period when drivers are less likely to be responsive to traffic rules, we should find that these rules are less ineffective. For example, it is intuitive that it would take a much larger increase in the beer tax to dissuade the average individual from drinking during happy hour than is necessary during normal work hours. On the other hand, during the Monday to Friday rush hour period, it is plausible to expect that the risk preferences of the average average driver to be closer to our core sample. We expect that this time period is mainly comprised of individuals travelling from work, which limits the potential effect that the migration bias is driving the main results. Under this conditions, it would be counterintuitive if any law(s) that the were insignificant in the main specification, are now found to be impactful.

Table 5 show the results of the falsification tests. Panels 1, 3 and 5 presents the traditional estimates, while panels 2, 4 and 6 present the estimates for the contiguous county-pair specification. As expected, during the peak periods of drunk fatalities, the contiguous county-pair specification finds that the laws were generally ineffective in curbing drunk driving fatalities. Plausibly, this approach also show that a BAC limit of 0.2g/dL for young drivers had a small effect in reducing drunk driving fatalities, while all other laws are statistically insignificant. In comparison, the traditional specification continues to find that a majority of these laws are effective during these periods. For legislation implementing a minimum fine for the first DUI, the tradition specification found the law to be ineffective in the main results, but the law becomes significant in a less sensitive environment. These anomoles can be taken as stylistic evidence that there is a bias towards finding an impact for traffic safety legislation using traditional state-level specifications.

Similarly, the results for the laws effectiveness during rush hour traffic confirms expectation when we utilize the contiguous county-pair specification. The laws that were found to be significant in the main results continues to be significant, but their impact reduces quantitatively. In contrast, the traditional specification suggests that most of the laws are insignificant and a few are even found to counter-intuitively increase the level of drunk driving fatalities.

On the one hand, these falsification tests show that given the main results, the contiguous county-pair specification influences individual behavior in a plausible and predictable manner under specific conditions. On the other hand, the traditional specification fail to consistently produce plausible estimates when used to examine the laws performance in restrictive environments. These results support the argument that there are major concerns about the robustness of the traditional specification.

4.4 Policy Implications

In this section, we provide back of the envelope calculations of the effectiveness of the various laws we found to be effective in reducing driving related fatalities. To be consistent with the usual statistics documented by the National Highway Traffic Safety Administration, table 5 provides the actual and potential effect of traffic laws in reducing total fatalities. The actual lives saved can be interpreted as the number of lives that would have been lost if the states that enacted the particular law, had decided not to do so. This is simply calculated as the beta coefficient from panel 4 or 8 of our regression model in table 4 weighted by the population in the states that enacted these laws. In contrast, the potential lives saved should be interpreted as the number of additional lives that would have been saved had the particular law been enacted in all states. Rows 1 to 2 of this table provides estimates of the total aggregated actual and potential lives saved over the period 1986-2005. Row 3 then shows the share of total lives saved to the total amount of lives the law could (actual plus potential) have saved over the period. Rows 4 to 7 then shows the actual number of lives that were saved by each laws in the select years 1990, 1995, 2000 and 2005 and rows 8 to 11 rows shows the additional lives that could have been saved in 1990, 1995, 2000 and 2005. Panels 1 to 5 shows the effect of each laws on the actual number of lives saved for states who enacted the law and the potential lives that could have been saved if the law was a federal mandate. Panels 6 and 7 shows the simulated effect of a proposal enacting a federal mandate which sets the speed limit on rural interstate highways to 60 MPH and a mandate which raises the beer tax rate to 40 cents per gallon in all periods under study. For these two panels, the estimates are done for the population that would be bounded by these policy mandates. Consequently, the states that enacted a rural highway speed limit below 60 MPH (more restrictive) or a beer tax rate above 40 cents per gallon would not be bounded by these mandates and are already saving a greater number of lives than if they were to adopt the mandate. By assuming that the states that are unbounded would not change their policies, we provide no estimate for the actual lives saved for these proposed mandates.

Over the period of study, the laws presented in table 5 saved approximately 147,000 lives in the states that they were enacted. Had these laws been enacted as federal mandates, along with a minimum beer tax and a maximum speed limit as outlined, then an additional 445,500 lives would have been saved. Consequently, policy makers are operating far below the pareto optimum, saving only 33% of the total lives that could be saved from fatal accidents with available legislative tools. With total fatal accidents accumulating to around 856,900 over the period 1986-2005, this suggests that these mandates would have prevented approximately 52% of these fatal accidents. Instead, policymakers only saved 17% of the total lives lost to motor vehicle accidents over the period.

5 Conclusion

In this paper, we present new evidence on the effectiveness of traffic legislation on the rate of traffic fatalities. To robustly evaluate this issue and to assess the empirical limitations of previous studies, the contiguous county-pair approach is employed. We argue that this approach offers significant insight into the causal effect of each policy because of our ability to directly address the issue of spatial heterogeneity that has biased previous estimates. Additionally, county-pair level analysis benefits from several empirical and efficiency advantages, that state-level panel and time series analysis do not.

Our findings suggest that the laws related to accident prevention, such as having a good graduated licensing system, pigovian beer taxes and primary seatbelt enforcement, are the most eïňĂective in reducing traffic fatalities. The empirical effectiveness of these laws suggest that they can employed as policy tools by policymakers to address the widespread public health issue of motor vehicle fatalities. Simple simulations suggests that policymakers have been utilizing existing traffic laws sub-optimally, saving only 17% of the lives lost to motor vehicle crashes.

While this study does not solve all the problems in the literature and may have limitations of its own, we are optimistic that it will be insightful to future efforts examining this question. In order for policymakers to implement policy aimed at combatting this phenomena, there is a need for robust studies exploring best practices that are effective. This study makes a single step in arriving at these solutions.

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6 Appendices

Table 3: Estimates of the Impact of Traffic Laws on Total Driving Fatalities

	State Level	All-Counties	County-Pairs	County-Pairs
	[1]	[2]	[3]	[4]
Good GDL Law	-0.93*	-2.29***	-3.12***	-2.01**
	[0.570]	[0.804]	[0.917]	[0.61]
Fair GDL Law	-0.39	-1.484**	-1.69***	-0.32
	[0.555]	[0.649]	[0.631]	[0.574]
Open Container Law	0.00062	-0.737*	-1**	-0.198
	[0.369]	[0.493]	[0.519]	[1.072]
Keg Registration	-0.27	-1.175*	-0.03	1.71
	[0.287]	[0.787]	[1.024]	[1.570]
Beer Tax Rate	-1.8	-10.49***	-13.54***	3.69
	[3.396]	[4.388]	[5.443]	[6.461]
BAC Limits	20.95**	8.346	15.87	-19.41
	[11.85]	[24.26]	[30.92]	[41.04]
Comm Service 1st or 2nd DUI	-0.0699	-0.35	0.17	0.46
	[0.361]	[0.458]	[0.486]	[0.516]
License Suspension 1st DUI	-0.627**	0.079	-0.62	-1.05**
	[0.304]	[0.707]	[0.571]	[0.521]
Prison Time 1st DUI	0.0316	-0.054	-0.19	-0.75
	[0.213]	[0.468]	[0.545]	[0.647]
Mandatory Min Fine 1st DUI	-9.62e-05	-0.00034	0.00018	0.000102
	[0.000792]	[0.00149]	[0.00139]	[0.00197]
Speed Limit Rural Interstate	0.0190	0.14***	0.15***	0.14**
	[0.0180]	[0.0490]	[0.0579]	[0.0598]
BAC limit of .02 g/dL for Young Drivers	0.172	0.46	0.48	0.2
	[0.242]	[0.439]	[0.637]	[0.904]
Bicycle Helmet Use Law	-0.6**	-2.05***	-2.35***	-0.44
	[0.274]	[0.551]	[0.613]	[0.733]
Law reducing comp. for SB nonuse	-0.792*	0.47	-0.33	-0.99
	[0.574]	[1.397]	[1.555]	[1.181]
SB Law: Minors	-0.876**	-1.8***	-2.19	-0.15
	[0.492]	[0.602]	[1.454]	[1.518]
SB Law: All	-0.786	-1.56**	-1.47**	-2.43***
	[0.683]	[0.699]	[0.651]	[0.702]
Max Fine SB nonuse	0.293	0.47	0.17	0.26
	[0.437]	[0.813]	[0.824]	[0.705]
Constant	-8.630	37.00***	377.2***	26.18***
	[12.42]	[13.64]	[78.16]	[9.560]
Controls	Yes	Yes	Yes	Yes
State FE	Yes	No	No	No
Time FE	Yes	Yes	Yes	No
County FE	No	Yes	Yes	Yes
County-Pair Fixed Effects	No	No	No	Yes
Observations	927	59,073	40,104	40,104
R-squared	0.902	0.467	0.46	0.73

Table 4: Estimates of the Impact of Traffic Laws on Drunk Driving Fatalities

	State Level [5]	All-Counties [6]	County-Pairs [7]	County-Pairs [8]
Good GDL Law	-0.59*	-2.12***	-2.6***	-0.88**
	[0.442]	[0.660]	[0.768]	[0.45]
Fair GDL Law	-0.23	-1.17**	-0.92**	-0.59*
	[0.351]	[0.559]	[0.48]	[0.44]
Open Container Law	-0.15	-0.88**	-0.81**	-0.861**
	[0.361]	[0.515]	[0.47]	[0.46]
Keg Registration	0.23	0.018	0.04	0.36
	[0.307]	[0.523]	[0.52]	[0.6]
Beer Tax Rate	-1.62	-6.70**	-18.5***	-10.74**
	[1.796]	[3.434]	[3.73]	[5.23]
BAC Limits	27.18**	23.57	12.57	-14.93
	[12.34]	[23.01]	[24.15]	[22.29]
Comm Service 1st or 2nd DUI	-0.136	-0.11	-0.22	-0.05
	[0.213]	[0.387]	[0.35]	[0.37]
License Suspension 1st DUI	-0.454	-0.82*	-0.19	-0.61**
	[0.446]	[0.568]	[0.5]	[0.37]
Prison Time 1st DUI	-0.298	-0.34	-0.32	-0.76*
	[0.275]	[0.435]	[0.46]	[0.46]
Mandatory Min Fine 1st DUI	-0.000134	0.00092	-0.0007	0.00006
	[0.000504]	[0.00110]	[0.0012]	[0.0013]
Speed Limit Rural Interstate	-0.0114	0.0098	-0.07*	-0.12**
	[0.0137]	[0.0262]	[0.0473]	[0.056]
BAC limit of .02 g/dL for Young Drivers	-0.353*	-0.66**	-1.29***	-1.48**
	[0.221]	[0.342]	[0.37]	[0.69]
Bicycle Helmet Use Law	-0.374*	-0.71**	-0.75*	-0.65
	[0.250]	[0.430]	[0.46]	[0.55]
Law reducing comp. for SB nonuse	0.709	2.09*	1.14	0.73
	[0.864]	[1.538]	[1.2]	[1.08]
SB Law: Minors	1.213*	1.41	0.4	.77
	[0.912]	[1.969]	[0.79]	[0.97]
SB Law: All	-0.12	-0.28	0.23	0.35
	[0.370]	[0.449]	[0.38]	[0.47]
Max Fine SB nonuse	-0.368	0.000087	-0.27	-0.34
	[0.544]	[0.852]	[0.65]	[0.64]
Constant	-9.529	20.98**	335.9***	32.66***
	[11.99]	[8.630]	[67.8]	[6.73]
Controls	Yes	Yes	Yes	Yes
State FE	Yes	No	No	No
Time FE	Yes	Yes	Yes	No
County FE	No	Yes	Yes	Yes
County-Pair Fixed Effects	No	No	No	Yes
Observations	927	59,073	40,104	40,104
R-squared	0.805	0.224	0.73	0.64

Table 5: Further Estimates: Effectiveness of Legislation For Select Periods

	Fri and Sat (12AM to 3AM) [1]	Fri and Sat (12AM to 3AM) [2]	Holidays [3]	Holidays [4]	Mon to Thurs (3PM to 6PM) [5]	Mon to Thurs (3PM to 6PM) [6]
Good GDL Law	-0.0344	-0.0274	-0.0658	-0.230	-0.0231	0.0454
	(0.0583)	(0.135)	(0.0572)	(0.216)	(0.0222)	(0.103)
Fair GDL Law	-0.0282	0.174**	0.00509	0.175	-0.0122	0.0463
	(0.0392)	(0.0767)	(0.0400)	(0.139)	(0.0147)	(0.0673)
Open Container Law	-0.0265	-0.0938	-0.0439	0.0458	0.00562	0.0529
	(0.0323)	(0.179)	(0.0422)	(0.129)	(0.0177)	(0.0677)
Keg Registration	-0.0196	0.0931	-0.0123	0.0350	0.0212	0.122
	(0.0327)	(0.176)	(0.0394)	(0.154)	(0.0183)	(0.0764)
Beer Tax Rate	-0.648***	-2.152	-0.365**	0.462	-0.0139	-2.125**
	(0.121)	(1.329)	(0.175)	(1.448)	(0.0710)	(0.941)
BAC Limits	1.915	-5.570	3.170*	-12.34	0.993	4.920
	(1.547)	(5.366)	(1.739)	(7.390)	(0.742)	(3.922)
Comm Service 1st or 2nd DUI	-0.0333	-0.107	-0.00746	-0.105	0.00435	0.00616
	(0.0314)	(0.117)	(0.0307)	(0.112)	(0.0116)	(0.0507)
License Suspension 1st DUI	-0.0207	0.132	-0.0461	-0.0127	-0.0226	-0.152**
	(0.0342)	(0.0939)	(0.0555)	(0.113)	(0.0243)	(0.0578)
Prison Time 1st DUI	-0.0191	-0.122	-0.0684**	0.0125	-0.0249	-0.0398
	(0.0271)	(0.0762)	(0.0278)	(0.184)	(0.0151)	(0.0540)
Mandatory Min Fine 1st DUI	-0.000127***	-8.3E-05	-4.81e-05	0.000562	2.60e-05	0.000434
	(4.48e-05)	(0.000425)	(6.24e-05)	(0.000556)	(2.67e-05)	(0.000367)
BAC limit of .02 g/dL for Young Drivers	0.000216	-0.0823	-0.0703*	-0.340***	0.0214	0.0971
	(0.0313)	(0.150)	(0.0385)	(0.123)	(0.0231)	(0.0612)
Bicycle Helmet Use Law	-0.0237	0.00773	-0.0616**	0.176	-0.0175	-0.101
	(0.0330)	(0.140)	(0.0257)	(0.149)	(0.0160)	(0.130)
Law reducing compensation for seatbelt nonuse	-0.0659	0.0716	0.148*	-0.0558	0.0854*	0.0857
	(0.0835)	(0.280)	(0.0771)	(0.273)	(0.0437)	(0.136)
SB Law: Minors	0.100	0.0598	0.0775	0.147	0.0640*	0.0433
	(0.109)	(0.372)	(0.109)	(0.385)	(0.0327)	(0.146)
SB Law: All	-0.0213	-0.151	-0.0275	-0.0741	0.0169	-0.150*
	(0.0491)	(0.123)	(0.0500)	(0.129)	(0.0147)	(0.0885)
Max fine seatbelt nonuse	0.000317	0.0363	-0.0795	0.140	-0.0347	0.0778
	(0.0455)	(0.137)	(0.0551)	(0.155)	(0.0310)	(0.0869)
Constant	-5.480***	6.983**	2.719**	3.596	-1.933**	0.120
	(1.873)	(3.431)	(1.351)	(2.935)	(0.751)	(1.174)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Prefered Spec.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	927	40,104	927	40,104	927	40,104
R-squared	0.617	0.541	0.553	0.538	0.451	0.535

Table 6: Effectiveness of Traffic Legislations in Reducing Motor Vehicle Accidents

	[1] SB Primary Enforce	[2] License Suspension	[3] Open Container	[4] Prison Time	[5] Good GDL	[6] Federal Max Speed of 60	[7] Federal Min Beer Tax of 0.40
Total Lives Saved	53916	50925	24810	9700	7650	0	0
Total Potential Lives Saved	85053	33241	17675	31026	70297	84572	123639
Total Lives Saved (%)	39	61	58	24	10	–	–
Lives Saved in 1990	1364	1834	829	328	0	0	0
Lives Saved in 1995	2422	2954	1152	356	0	0	0
Lives Saved in 2000	3980	3376	1365	598	191	0	0
Lives Saved in 2005	4875	3785	2221	952	1921	0	0
Potential Lives Saved in 1990	5437	2285	1250	1665	4798	2359	6319
Potential Lives Saved in 1995	4833	1440	1066	1771	5118	4344	6365
Potential Lives Saved in 2000	3705	1279	985	1654	5231	6187	6628
Potential Lives Saved in 2005	3240	1130	260	1426	3804	6752	6982